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Reply to the Letter to the Editor

Reply to the Letter to the Editor by Maunder

The major modeling conclusion of Prager (2002) was that the generalized production model (Pella and Tomlinson, 1969; Fletcher, 1978), when its shape parameter is estimated with other model parameters, is unduly influenced by outliers. For that reason, I recommended use of a fixed model shape as preferable, with the logistic model (Schaefer, 1954, 1957) forming a central approximation when no estimate of shape can be made externally. The shape parameter in this context is the ratio $\phi = B_{\mathrm{MSY}}/K$, where B_{MSY} is the stock biomass from which maximum sustainable yield (MSY) can be obtained, and K is the maximum biomass or carrying capacity of the stock.

For the most part, Maunder's letter uses Prager (2002) as a springboard for proposing that the logistic production model be discarded, rather than responding directly to the material in Prager (2002). Maunder's proposal rests on two main assertions: (1) "the logistic model should never be used, because it can be represented by the generalized model with the shape parameter fixed at 0.5," and (2) "[The logistic model] is not appropriate for most stock assessment applications, and a generalized model with the shape parameter fixed at somewhat less than 0.5 would be more appropriate." Here, I respond to both assertions.

I am somewhat puzzled by the first assertion. Because a model is simply a mathematical construct, the logistic model is identical to (not "represented by") the generalized model with fixed shape parameter of $\phi = 0.5$. If Maunder's point is that an analyst's thinking should not be limited to the logistic model, I agree, and nothing in Prager (2002), if read carefully, should imply otherwise. Indeed, all fishery models are approximations, and consequently many analysts prefer using more than one model to analyze a stock, as does, e.g., the ICCAT species group for swordfish (Prager, 2002).

Besides disagreeing with his proposal, I object to Maunder's use of my work in two specific instances. First, he states that the shape estimate of McAllister et al. (2000) (of which I am coauthor) is "more realistic" than the central approximation $\phi = 0.5$. Without criticizing McAllister et al. (2000), I must note that it is one of two gray-literature papers that considered the question and reached opposing conclusions, the other being Garcia-Saez (1997). Second, Maunder's Fig. 1 distorts an analysis in Prager (2002). My analysis followed the procedure of Rousseeuw and Leroy (1987) for removing outliers from a data set and refitting a model. Maunder distorts the method by removing outliers based on a logistic model (a model he dismisses) and then fitting a different (generalized) model to the remaining data. I believe that in removing outliers it is vital to follow a consistent method devised a priori to avoid any suggestion that methods are chosen to arrive at some preferred result.

Maunder's letter includes a series of numbered arguments. I examine those arguments next.

(1) *It is unlikely that the same model shape would be appropriate for modeling both numbers and biomass*. The practical effects of modeling numbers or biomass must be evaluated for each species. This question was addressed to some degree by Prager and Goodyear (2001), who found that mixing numbers and biomass in the same analysis of a blue-marlin-like simulated stock caused only moderate increase in variance. Maunder's argument does not weaken the usefulness of the logistic model as an approximation to other possible shapes.
(2) *Some management advice is sensitive to the model shape used. ... in* Prager (2002) *the ... population is substantially overexploited. However, if* [*another*] *value of the shape parameter ... is*

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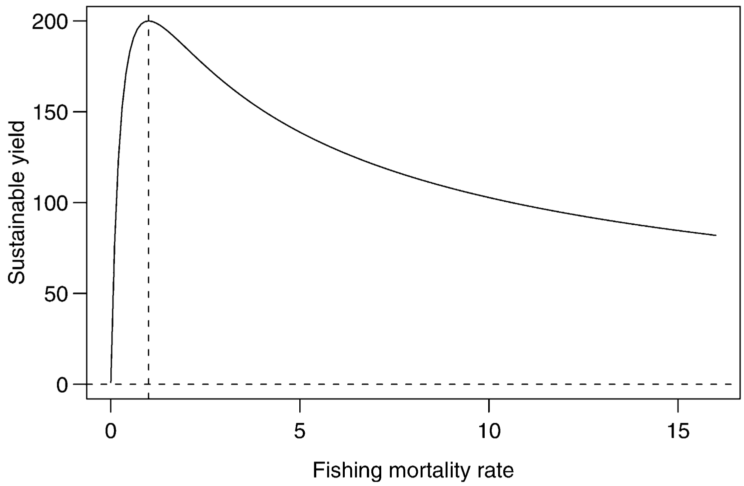

Fig. 1. Sustainable yield as a function of fishing mortality rate for a population that follows the generalized production model with $\phi = 0.2$, MSY = 193 units/year. Vertical dashed line locates $F_{\mathrm{MSY}} = 1.0$ year.

used, the population is only slightly overexploited. Management advice can be sensitive to the value of ϕ assumed, just as to many other assumptions on model structure. For that reason, the discussion section of Prager (2002) recommended that analysts conduct sensitivity analyses on model shape.

Sensitivity of management advice to the value of ϕ is (as Maunder states) greatest when the initial stock biomass must be fixed relative to carrying capacity, a procedure used to overcome shortcomings in the data. For example, in the swordfish analyses of Prager (2002), the initial years of the abundance index appear unrealistic. Such problems cannot be resolved without better knowledge of the processes that generated the data (Prager, 2002). Because we lack such knowledge, robust fitting techniques were used in Prager (2002) to avoid the problem. In such cases, data failures cannot be rectified by a different choice of model shape.

Finally, it is easy to spend too much energy debating whether a stock is "substantially overexploited" or "slightly overexploited." Theory predicts that in either case, a reduction in fishing effort will increase population size and stability and increase sustainable yield. Results of successive assessments can be used in an adaptive approach to refine the best level of reduction.

(3) *Various models and analyses can be used to estimate the shape of the production function*. That ϕ sometimes can be estimated does not reduce the usefulness of the logistic model when such estimates cannot be made, nor does it support preference for $\phi < 0.5$ as a default. Estimating the shape of the production curve demographically will depend on the selectivity vector, maturity vector, and their relationships; growth patterns; recruitment patterns; and on what portion of the population is sampled by the abundance index. For many stocks, sufficient data are not available for a demographic approach. Moreover, every such estimate of ϕ relies on specific model assumptions, and the degree to which an actual population follows those is rarely known (Prager, 2002). For that reason, one might want to think carefully before adopting an estimate that says a population should be reduced to 11% or even 23% of its virgin size to obtain maximum yield, as in Maunder's Table 1. (That table, the reader should note, is based on steepness (productivity) values only in the upper part of the possible range, including the theoretical maximum value, $h = 1$.)

(4) *One problem with production models is that they do not model age structure of the population and therefore cannot incorporate changes in selectivity. . . .* Production models are hardly

unique in being approximations. For example, catch-at-age models generally ignore compensation in growth or natural mortality rate, both of which may be handled implicitly by production models. Because appropriate management advice generally depends on selectivity, Prager et al. (1996) examined changes in selectivity for swordfish and concluded that the observed changes were not problematic in production modeling of this stock.

(5) *Why would the logistic model . . . be the appropriate . . . default . . . for fish populations? Why not the Fox model . . . ? I suggest that the default model should be the generalized model with its shape parameter set at a value . . . consistent with . . . the population*. Obviously, model assumptions should be as consistent as possible with the population under study. However, when data are not sufficient to estimate ϕ satisfactorily, the logistic model is the customary default because of its basis in linearity, which leads to its selection by Occam's razor. It also enjoys over 100 years of historical precedence (Kingsland, 1982), which means that its properties have been well studied. Whether a different shape would make a better default, and what that shape should be, remain open questions. The important results of Thompson (1992) cannot be taken as a blanket prescription because some real data sets undoubtedly vary from Thompson's modeling assumptions. Maunder's suggestion to use conventional hypothesis tests or likelihood values to choose model shape is subject to the problems described in Prager (2002). Briefly put, such tests underestimate the likelihood of obtaining outlying values in abundance indices and thus are unduly influenced by such outliers. Maunder's emphatic reiteration of the assertion $\phi < 0.5$ does not lend it support, nor does it define a new default value for ϕ, nor does it reduce the usefulness of the logistic model as an approximation.

(6) *Why not just use an age-structured production model*? This question is irrelevant to the topic of Prager (2002), and even to Maunder's own proposal. It seems best to me to use a variety of models if at all possible.

Having addressed the numbered points, I now briefly discuss a few more general issues. Although one can derive a production curve from age-structured analyses, that curve is not the same as the generalized (Pella and Tomlinson, 1969) production curve with the same shape parameter. Furthermore, the applicability of either curve to a real population depends on the fishery's meeting the assumptions, particularly those on selectivity. For example, consider a curve of sustainable yield plotted against fishing mortality rate for a population following the Pella–Tomlinson model with $\phi = 0.2$ (in the range of Maunder's estimates for yellowfin tuna). That value of ϕ implies that sustainable yield remains high even when the fishing mortality rate is 15 times the rate at MSY (Fig. 1). Before adopting such an extreme premise in management, one should be certain that the model's assumptions are valid and explore sensitivity to factors such as unreported bycatch of supposedly unselected fish, or environmentally driven recruitment variability. Using an estimate (ϕ) from one type of model to specify another model entails making strong, and not always obvious, assumptions about population behavior. If model assumptions are violated, there can be deleterious consequences to the stock, especially a stock of moderate productivity. It is desirable that management recommendations from fishery models be robust to data errors, and when ϕ is assumed small, model robustness is likely to suffer.

Rather than rejecting the logistic model, I believe that research into many types of population-dynamics models must continue, and that fishery education should include exposure to a wide variety of models, from simple to complex. Most importantly, students must be taught that each model provides, from its particular perspective, an imperfect view of reality. The more perspectives one can gain, the better. To answer the argument that students are able to apply only the logistic model, I have made available on my Web site (http://shrimp.ccfhrb.noaa.gov/~mprager/) software (ASPIC 4.4 and later) for use by those who wish to try the generalized production model, with fixed or estimated shape, without programming it themselves.

In practical terms, the logistic model can be useful in analyzing data-poor stocks, for which ϕ usually cannot be estimated with confidence. Data-poor stocks are typically subject to outliers in the abundance index, because they are poorly sampled. Furthermore, an important component of setting a default $\phi \neq 0.5$ is specifying the default value that ϕ should take, not just the value it should *not* take. That issue is avoided by using the simpler logistic model, which when ϕ is sufficiently close to 0.5 provides a good approximation. For any stock analyzed with a production model, it will be useful to conduct sensitivity analyses of management recommendations to assumptions on model shape. It is no less important to examine age-structured models for sensitivity to assumptions.

In conclusion, Maunder argues against the paper I did not write, one that puts forth the logistic production model as a panacea. A closer reading would reveal that the topic of Prager (2002) is the instability of estimates from the generalized model with free shape parameter. Maunder and I apparently agree that in using a production model, the best case will be to supply the shape parameter a priori. I am probably less sanguine than he about direct use of demographic estimates of ϕ in standard production models, which for low values of ϕ exhibit excessively high per-capita productivity at small population sizes. Using such estimates with alternative production model forms (e.g., McAllister et al., 2000) seems more promising. A point on which we disagree is the strength of evidence that the default shape of the production curve is best represented by some value $\phi < 0.5$. Maunder apparently believes that the evidence is conclusive. I find the work of Thompson (1992) (and others) intriguing, but I believe that the jury is still out on the best assumption to use in a management context. I suspect that increasing evidence (which should include simulations on imperfect data sets) will help resolve this question in the coming years. It is through such disagreements between conservatism (which values what is well known and understood) and advocacy of change (which values the potentially better, but less fully understood) that progress in our field is made.

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